of the Root." Dr. Frank "On the position and direction of floating and submersed parts of plants." Dr. Cohn "On Parasitic Algæ." Dr. Schroeter "On certain Pigments formed by the Bacteriæ," and Dr. Cohn's "Investigations on Bacteriæ."

This work—Cohn's "Beiträge zur Biologie der Pflanzen"—is the organ more especially of the workers in the phyto-physiological laboratory at Breslau, under the guidance of the eminent Professor of Botany in the University there. The first part appeared in 1870 (NATURE, vol. iii. p. 242).

Dr. Ciesielski's researches follow up the interesting inquiries into the same subject opened a few years ago by Prof. Hofmeister and others. He sums up with the following propositions:—

1. The normal growth of germinating seedlings may be observed in a saturated atmosphere, if the albumen or cotyledons be kept constantly moist, without the root descending into either water or moist earth. The development of the root ceases however as soon as the reserve of nutrient matter in the seed is exhausted.
2. The longitudinal extension of the root takes place exclusively in a relatively narrow region behind the tip.
3. The downward curvature of the root takes place at that point where the longitudinal growth of its cells is at a maximum.
4. Gravitation occasions the downward curvature.
5. This curvature is not passive but active; that is to say, gravitation occasions in the root a tension of the tissue whenever it departs from its normal direction, and thus determines downward curvature.
6. This tension is due to the more considerable growth of those cells which lie on the side of the root turned towards the zenith.
7. The more marked growth of the cells of this side of the root is occasioned by the circumstance that the cell-contents of the upper side turned towards the zenith are much less concentrated than in the under side; which, again, is due to the action of gravity which determines that the more concentrated cell-sap, being the heavier, shall occupy the under side of the root.
8. If the outermost tip (*Vegetationskegel*) of a root be cut off, it may indeed elongate through mere extension of its tissues, but it is no more capable of downward curvature.
9. If however it afterwards developes a new formative apex, as occasionally happens, and so elongates at the cut apex, the root again becomes capable of downward curvature.
10. Centrifugal force determines in a similar manner and on analogous ground, the curvature of the root in the direction of this force, as does gravitation in that towards the nadir.

The next thing will be to explain the relation of *ascending* axes to gravitation. It would seem as though heliotropismus and geotropismus had squabbled over the embryo and compromised matters at last by each taking a *Vegetationskegel*, one of the plumule the other of the radicle, with its active future control!

Dr. Frank's paper is an inquiry into the causes which influence the position and direction of the floating and submersed leaves of aquatic plants apart from the direct operation of gravity and sunlight, which are not causes directly productive of special modifications of growth, but rather guides according to which the organ in the course of growth adjusts itself so far as it is able, until it has attained a position the most advantageous to it.

Prof. Cohn's memoir touches upon the question, of peculiar interest just now, of the relation of the lower forms of vegetable life destitute of chlorophyll to the organic matter in which they find their matrix and to the inorganic world, with regard to their power of assimilating independently their necessary nutriment and reducing carbonic acid. He also carefully describes as a new unicellular genus of truly parasitic algæ, *Chlorochytrium* (allied to *Hydrocytium* and the *Chytridinæ*) which he finds inhabiting the fronds of *Lemna trisulca*. The occurrence of this chlorophyll—containing alga in *Lemna*, he regards as presenting an analogy with the presence of the green gonidial layer in lichens, which he looks upon similarly as an endophytal alga.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Janssen-Lockyer Application of the Spectroscope

I SEE by a letter in the *English Mechanic and World of Science* of January 31, that Mr. R. A. Proctor claims for Dr. Huggins the credit of having publicly described in Feb. 1868, the method of viewing the solar protuberances without a total eclipse.

Permit me to state that to Mr. Lockyer undoubtedly belongs the credit of having known this method even before his preliminary paper in 1866 in which it was sufficiently published. We had numerous conversations on the subject, and when he showed me the MS. of his preliminary paper and told me at the same time that he was about to ask a grant for the purpose of procuring a more powerful spectroscope, I advised him to introduce his views in the shape of a question, which he accordingly did in the following terms:—"May not the spectroscope afford us evidence of the existence of the 'red flames' which total eclipses have revealed to us in the sun's atmosphere, although they escape all other methods of observation at other times? and if so may we not learn something from this of the recent outburst of the star in Corona?"

I gave this advice to my friend, Mr. Lockyer, because I thought that as it might be some time before he obtained the new instrument it might be well that he should publish what I conceived would enable him to claim for himself the knowledge of this principle. And I think that anyone well acquainted with spectra on reading the question put, could not fail to see what was meant, and if he were previously ignorant of the principle, he could not fail to perceive it. I therefore feel rather astonished that anyone should claim the statement made by Huggins two years afterwards as being the commencement of a new principle. The method founded upon this principle was first successfully applied by Messrs. Janssen and Lockyer, acting independently of each other, in the year 1868. Mr. Lockyer had by this time obtained his improved spectroscope, and the very first day he used it he made the discovery. I do not think there is any reason to suppose that Mr. Lockyer was at all aided, as was suggested by Mr. Huggins, in detecting these lines by the somewhat vague information of what had been done at the total eclipse in India. He was now provided with an instrument capable of showing these lines to a child—made in fact for the purpose of showing them. Now what would have been thought of any one of the Indian observers if during the few minutes of a total eclipse he had failed to detect the existence of bright lines? Can we imagine therefore that an intelligent observer like Mr. Lockyer, with an instrument constructed for the purpose and with unlimited time at his disposal, should have failed in detecting such lines even had he not previously been sure that they existed? I confess I cannot understand any observer failing in a thing of this nature, and therefore I do not know well how to account for the somewhat puzzling remark made by Dr. Huggins in his notes to Schellen's "Spectrum Analysis" as follows:—

"Though to Mr. Lockyer is due the first publication of the idea of the possibility of applying the spectroscope to observe the red flames in sunshine, as a matter of history it should not be passed over that, about the same time, the same idea occurred quite independently to two other astronomers, Mr. Stone of Greenwich, and Mr. Huggins. These observers were however unsuccessful in numerous attempts which they made to see the spectra of the prominences, for the reason probably that the spectroscope which they employed was not of sufficient dispersive power to make the bright lines of the solar flame easily visible. When the position of the lines was known, Huggins saw them instantly with the same spectroscope (two prisms of 60°), which he had previously used in vain."

I confess I cannot yet understand why a distinguished observer

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like Dr. Huggins, possessing such an instrument as he did, should fail to have seen the bright lines at first, nor why, believing as he did in the aid afforded by the Indian observations to an observer searching for these lines, he should yet have left it to Mr. Lockyer to make this discovery.

BALFOUR STEWART

Dr. Bastian's Experiments on the Beginning of Life

IN the issue of NATURE for January 9, Dr. Burdon Sanderson has recorded some experiments on the behaviour of certain organic mixtures boiled for five or ten minutes in flasks which were hermetically sealed during ebullition. He found, as Dr. Bastian had done, that Bacteria appeared in these sealed vessels—not always, but frequently. Dr. Sanderson is, however, careful not to endorse the conclusions which Dr. Bastian has drawn from these experiments. This method of experimenting appears to me to involve two serious sources of error which invalidate them in so far as they are used to support the theory of spontaneous generation.

The first source of error is the possibility of the introduction of atmospheric germs at the moment of sealing.

Those who are practised in the sealing of flasks during ebullition are aware that the sealing can only take place just as ebullition is about to cease; otherwise the vessel bursts, or the imprisoned steam opens a path for itself through the softened glass where the flame is being applied. There is thus at the moment of sealing, a risk of some reflux of air into the flask and a consequent vitiating of the experiment. Perhaps it may be thought that because air thus introduced has to pass through the flame applied to the tube through the red-hot tube itself, and enters a flask whose contents are not far below the boiling temperature, any germs contained in it must be destroyed; but that is a very hazardous assumption. *Momentary* contact (or approximate contact) with a flame or a heated surface is by no means so destructive as at first appears, and experience has taught me to suspect that the contents of a boiling flask are not speedily deprived of vitality. This source of error is probably not a frequent occurrence in Dr. Bastian's experiments, but I am inclined to think that it was a frequent one in Dr. Wyman's experiments, and that it seriously vitiates all those experiments in which air passed through a heated tube was used. At any rate the exactness of experiments so conducted is very much at the mercy of the care and dexterity of the operator, and hence, probably, their contradictory results in different hands.

In repeating Dr. Bastian's experiments I have avoided this source of error by inserting a tight plug of cotton wool in the neck of the flask before beginning to boil. In this way any chance germs introduced by an accidental reflux of air during sealing are prevented from passing into the flask.

The second source of error is much more important. It is this:—*Dr. Bastian's process does not insure that the entire contents of the flask are effectively exposed to the boiling heat.* Herein lies, I believe, the chief cause of the inconstant and contradictory results obtained by him. It is beyond doubt that Dr. Bastian is perfectly correct in his statement that the experiments made by Pasteur with "Pasteur's solution," and by Lister with urine, yield different results when made with other solutions and mixtures. The contrast is most striking. In my own experiments I have found that filtered infusions of any animal or vegetable substances (and I have tried a very great variety) can be invariably preserved unchanged when boiled for five or ten minutes in a flask plugged with cotton wool; but if milk be treated in the same way, or if a few fragments of a green vegetable be added to the infusion, or if alkaline albuminous solutions or mixtures, containing cheese, be treated in the same way, they almost invariably breed Bacteria in abundance. What is the cause of this difference? For some time it appeared to me difficult to account for, but I came to the conclusion at length that it was simply due to the fact, that with the more complex organic mixtures every particle of the material within the flask does not really attain the boiling heat. These more complex mixtures generally froth excessively in boiling, and spurt about particles which adhere to the glass, and probably some of these escape the full effect of the heat. What first led me to this conclusion was the behaviour of milk. Milk boiled for ten or even twenty or thirty minutes, in a plugged flask, almost invariably curdled and produced Bacteria in a few days; but when the milk was put into a long-necked flask, plugged with cotton wool, and hermetically sealed, and the flask boiled in a good-sized can of water for twenty or thirty minutes, then the milk remained permanently unchanged, and produced no Bacteria. I possess specimens of milk treated in this way which have remained unchanged for many months, though exposed to warmth, to light, and free access of air, that is to say, to air filtered through a good plug of cotton wool. I obtained similar results with the other organic mixtures which could not be kept unchanged by simple boiling over the flame. Highly putrescent mixtures, containing blood-serum, egg-albumen, fragments of meat and vegetables, remained perfectly barren after the flask containing them had been immersed in a water-bath kept at a boiling heat for twenty or thirty minutes.

The essential conditions of the experiment are, first, the effective exposure of the whole contents of the flask to a boiling heat; secondly, the absolute prevention of any fresh entrance of extraneous solid or liquid particles; and the conclusion I have come to is that if these conditions are rigidly observed, the flasks remain barren; if they do not remain barren it is simply because one or other of these conditions has not been observed.

Manchester WM. ROBERTS

The unreasonable

I UNRESERVEDLY accept Prof. Clifford's disavowal of the meaning I attributed to his words concerning Kant's Antinomies, in his Address (*Macmillan's Magazine*, Oct. 1872). At the same time I cannot allow that the misprision was wholly due to my "exuberant imagination." He said, "The opinion . . . is set forth by Kant . . . in the form of his famous doctrine of the antinomies," &c. This ought to mean that the "doctrine of the antinomies" is one form of that "opinion;" and the opinion being, "that at the basis of the natural order there is something which we can know to be *unreasonable*," I was fully justified by the mere words of the Address in the inference (which he disclaims) that he intended to identify the doctrine of the antinomies (the Antithetic, in fact) with that of the unreasonable basis of the natural order. How was I to know that the "something" was either (? which) "the transcendental object" or the world of *noumena*?

I premise, then, that it is the Antithetic which "is set forth by Kant in his famous [but little understood] doctrine of the Antinomies," and *not* "the opinion that at the basis of the natural order there is something which we can know to be unreasonable." Prof. Clifford, however, meant to signalise the latter; and he asserts, and by sundry extracts from the *K. r. V.* attempts to substantiate the assertion, that "the transcendental object [which lies at the basis of the natural order] is *unreasonable*, or evades the processes of human thought."

Now Kant, so far from proving (or asserting) *that*, takes pains to show that it is *reasonable*, though it persistently seems to be the reverse! According to Kant, the thing *per se* illusorily appears to be the object of experience; and this illusion is inevitable, and no criticism can dispel it. (Kant compares it to the seeming magnitude of the horizontal moon.) But criticism can and does explain it, so that, though it persists as a spectre haunting the reason, it is wholly and strictly amenable to the processes of reason, or in Prof. Clifford's sense, *reasonable*.

Of Prof. Clifford's quotations, (*a*) and (*b*) are irrelevant to his second position; the former does not directly touch "the processes of human *thought*;" the latter does not touch "the transcendental object!" His third position is equally unsupported by the extract, "Man [*not* Mann] kann aber," &c., which may be thus rendered:—"But conversely we can also deduce from this antinomy a real, not indeed a dogmatical, but a critical and doctrinal advantage, namely, of indirectly showing the transcendental ideality of phenomena (*Erscheinungen*)." The method is by showing that the antithesis is contrary, as distinguished from contradictory, and by invalidating both the alternatives, whence it follows that the subject of them is not an existing totality. The antinomies are thus used, not as Prof. Clifford vainly imagines, to prove that the transcendental object is unreasonable, but that the postulate of its being a *noumenon*, or thing *per se*, or true basis of the natural order, is untrue, both alternatives being false.

Prof. Clifford is, as I said, really attacking Hamilton. I do not care where he got the doctrine from, nor what he does with it. If it amuses him to set up these absurd nine-pins and then bowl them over, with flourish of trumpets, I have no wish to interfere with him, only he had better mind his H's and K's, and not impute this stuff to Kant. Once for all: in Hamilton's

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